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Social Science Research Findings and Educational Policy Dilemmas: Some Additional Distinctions

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Abstract

The article attempts to raise several distinctions regarding the presumed relationship of social science research findings to social policy making. The distinctions are made using Glymour's critique of the *Bell Curve*. An argument is made that (1) social science models and research findings are largely irrelevant to the actual concerns of policy makers and (2) what is relevant, but overlooked by Glymour, is how ideological factors mediate the process. The forms that ideological mediation may take are indicated.

Although there have been a variety of attempts to understand how social science research does or does not affect the "voices" of those being studied (Harding, 1993; Longino, 1993), we wish to revisit the issue from another angle. What has been overlooked in even the most ambitious constructivists' forays (Fuller, 1988) into dominant epistemologies is *why* such research findings are, generally, so overwhelmingly ineffective in social policy formulation. That is, we wish to consider some of the deeply implicit notions of the "research act" (Denzin, 1989) itself; those that

contribute to either the tacit acceptance of such knowledge production or generate vociferous attacks (Lakatos, 1978) of various sorts. More specifically, our argument is that social policy makers assume an atypical "gatekeepers" role where, in this case, they must attempt to appropriate, translate, and filter social science research findings to relevant publics; however, the very act of doing so is most likely doomed to fail. Those who are then to "benefit" from the social policies, informed and enlightened by social science findings, are the very ones whose voice often cannot be heard.

The issue is, to use Quine's (1969) overworked phrase, one of an "indeterminacy of translation." It is not that a translation is impossible, however, but rather that something is lost *in* the translation. What is lost is the subject of our analysis, including an attempt to show—again borrowing from Quine (1960)—that there is indeed a "fact of the matter" about all of this, but an unexpected one. We will attempt to show how the "translation" issue works by using the recent analysis of the well known philosopher of science, Clark Glymour, to account for the relationship of social science research, to social policy, to social practice. Specifically in his provocative article, "What went wrong? Reflections on Science by Observation and *The Bell Curve* (1998:1-32), Glymour recognizes the issues of evidence and policy relevant to both the philosophy of science and social science and how they overlap into the ambiguous realm of public policy-making. However, the need for additional analysis lies not only in the fact that Glymour has not fully explored a series of mostly implicit, but very significant, assumptions that are involved in social policy making, but also to illustrate that the nexus of scientific thinking and the formulation of social policy often support ideologically-based belief systems that selectively utilize "scientific" findings. Our aim will be to illustrate how even a well-known philosopher such as Glymour falls victim to the very trap he is trying to expose and avoid.

To begin with, Glymour's critique of the methodological (and in a deeper sense, ontological) issues he raises concerning the analysis of *The Bell Curve* (1994) are arguably some of the best made to date. The social sciences, Glymour argues, have been plagued by the alleged importance of uncovering the causal mechanisms underlying social behavior and practices. This is not a new problem. What is important, as he points out, is the inability of the social sciences to acknowledge that these implicit causal structures are highly complex, and being so, how they can produce contradictory conclusions within a given research domain. The complexity of these causal structures is often overlooked by social scientists because of implicit beliefs concerning the validity of the methodological techniques themselves (Campbell, 1987). For instance, if a social scientist can employ such relatively powerful quantitative techniques as multiple regression, discriminate analysis, and factor analysis, there are usually two corresponding beliefs that seem to come into play: (1) that such techniques take precedence over "philosophical" beliefs concerning the nature of (and presumed importance of) causality, and (2) the use of such techniques, irrespective of their ability—or lack of—to uncover true causal structures, still *improves* the claims that can be made about social behavior over-and- above what could be said in their absence. Again such debates, as Glymour correctly points out, mistake the importance of clear causal thinking with the technical application of methods.

He states the issue (p. 1):

Social statistics promised something less than a method of inquiry that is reliable in every possible circumstance, but something more than sheer ignorance; it promised methods that, under explicit and often plausible circumstance, converge to the truth, whatever that may be, methods whose

liability to error in the short run can be quantified and measured.

Glymour further correctly points out (pp. 2-3) that social scientists are still under the sway of a certain form of positivism that is suspicious of causal analysis itself. For him, there is a solution: "Clear representation by directed graphs of causal hypotheses and their statistical implications, in train with rigorous investigation of search procedures, have been developed in the last decade in a thinly populated intersection of computer science, statistics and philosophy" (p. 3). However, even this solution, potentially elegant as it is, in our view, will not provide the needed framework for rational social policy making. We will try to address why this is so in the sections that follows.

I.

To put the issue rather crudely, for those engaged in the policy making process what Glymour envisions, "just doesn't matter!" What we mean by this is that in social policy making, at many levels and across a variety of contexts, the discovery and justification of elegant (or even elementary) causal processes is largely irrelevant to the decisions made by policy makers. Part of the problem, to begin with, is the fact that there is what we will call an "ontological bifurcation" between social scientists and policy makers (who are usually not social scientists). These two groups—at least based on our own experiences—simply view the "world" in different ways, and often in such fundamentally different ways, that although they *want* to communicate often they cannot because, ultimately, they are unable to do so. While the story of why this is so is rather complex, Fuller's attempt to explain it is relevant here. He wrote (1988), for example,

Unfortunately, as our remarks were meant to suggest, *the crucial epistemological differences occur at the level of the different textual embodiments*, since a popularization of quantum mechanics offers the lay reader no more access to the work of the professional physicist than a state-of-the-art physics text offers the professional physicist access to the general cultural issues which interest the lay public. [His emphasis.](p. 272)

There are indeed different "textual embodiments" that are at the heart of the issues, but for us the policy maker-as-gatekeeper role is the crucial one to consider. This role serves as the principle "translator" one, mediating between the social scientist-as-researcher and the voices of specifically involved publics. In contrast with Fuller, however, we see the issue as primarily "ontological", although heavily conditioned by the epistemological. By this we mean, the issue of increased technique-sophistication, along with the causality issue, is *believed* to be necessary (and possibly sufficient) for an increasingly satisfactory and accurate "ontological-representation" of what social science research findings *can* do. We are suggesting, on the other hand, that the very belief in what social science can do for social policy making is at the center of differing views of (social) reality between these two groups, leaving aside the affected publics. One initial way of capturing the difference is to begin with a few "themes" about evidence that figure into the debate but are often not explicitly indicated as such. These themes are fundamentally about what constitutes "good" evidence for (eventually) the making of "good" policy, or about how differing textual embodiments come about.

Theme 1: "What is your evidence?"

From the policy maker's side of the ontological divide, the pressing issue is to be able to "take and use" the evidence of social science research, with methodological finesse(ness) be damned. Moreover, this is often the case for policy makers who are trained as social scientists. The issue of the evidence theme takes various forms. Perhaps, the most central one centers around the following distinction: "What evidence counts?" vs. "What counts as evidence?" The distinction is one with a difference, as we see it. Taking the latter one first, what counts *as* evidence includes a large class of possibilities, such as empirical and non-empirical (i.e., qualitative), historical, legal data, and so forth (Miller & Safer, 1993). Any of these *types* of evidence may be deemed to be relevant by the policy maker in terms of formulating, implementing or evaluating a given social policy. (Note 1) The issue is not trivial since how it is addressed, and by whom, can determine a wide range of decisions affecting peoples lives in terms of what voices they may or may not eventually have.

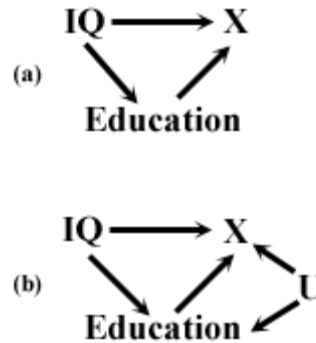
What is crucial to see, however, is how choices as to what does *not* count as evidence automatically entail *what* evidence counts. Thus, if we reject the use of, for example, ethnographic findings as evidence for a social policy issue, and our only other choice is some type of empirical evidence, then the process of elimination dictates the epistemological choice of what evidence counts. Here we may find a great deal of variation: experimental vs. correlational findings, for instance, and both further delineated by way of causal robustness. Moreover, each type of evidence may be further distinguished by such factors as "weight" and "number". Thus, the "weight of the evidence" may be a function of how "much" there is of it and how these concerns are counterbalanced by "internal" factors such as sampling strategies and numbers, parametric vs. non-parametric measures, the putative validity and reliability of measures used, their "normal distribution", and so on.

All of these considerations need to be, but seldom are, taken into consideration by the policy maker. Or, more precisely, even when they are their eventual impact on the policy making process is usually minimal.

Theme 2: "Do you have a causal model?", or "Does your data give rise to or support a pre-determined causal model?"

In many policy making scenarios, Theme 2 may or may not be related to *Theme 1*, and this from either side of the ontological divide. Social scientists who serve as (adjunct) policy makers in their role of "experts", based on our experience, seldom, if ever, *explicitly* engage in discussions of the causal robustness or the efficacy of their models. At best, such attempts are ad hoc; even where publication in empirical social science journals is concerned, the issue of "causality" is usually given the obligatory conceptual "nod" but then quickly forgotten. From the view of the non-social scientist policy maker the issue is moot, since it is usually so far divorced from what *needs* to be accomplished, it is perceived as irrelevant.

However, where a causal model could be specified with the precision argued for by Glymour, the implications for policy making are probably not as dramatic as he makes them out to be. Consider his two models (pp. 16-18, figures 12 and 13, respectively) as examples.



In (a), Herrnstein and Murray's (1994) model, IQ is the presumed cause of X (let's say some outcome variable), and while Education may "intervene" or "mediate" the IQ -- X relationship, something the social scientist would want to know, Glymour argues the "answer" to (a) may be mistaken because of the inability to account for the possibility of "U" in case (b). The "U" (e.g., "latent factors", other unknown "variables") may themselves be correlated with X and Education and hence give a false picture of what is presumed in (a).

Now, both (a) and (b) are examples of models that "count". Let's also assume that (b) is somehow fully specified and with "U" accounted for the role of Education is either enhanced or drastically reduced (i.e., in terms of explained variance). What is the social scientist-as- policy maker and policy-maker-non-social-scientist to make of this *for* policy purposes? The first may examine the total amount of variance explained (i.e., R^2), with or without the underlying causal structure, as not being *that* relevant. By this we mean, the social scientist *as* policy maker may: (1) judge (b) to be a "better" causal model because when "U" is taken into account the overall percentage of variance explained in X is "greater" than in (a), (2) maintain faith in (a) because the amount of unexplained variance (i.e., $1 - R^2$) has not been "sufficiently" reduced in model (b), or (3) perhaps "go with" (a) or (b) depending on what "U" is determined to be. If U is something like the mysterious "g-factor" for ability, as opposed to a more "straightforward" variable such as, hypothetically, "Parental Attitudes", the decision may be to stick with model (a) because it is putatively more amenable to policy making. On the other side, the non-social scientist policy maker (even given some understanding of the technical issues) still needs to know *what* to do—and (a) or (b) will not be very useful here. Why not?

One reason is that the policy maker (perhaps of either variety) is engaged—although most likely implicitly—in the formulation of a *practical argument*; one, roughly, similar to Aristotle's (*DeMotu Animalium*, Ch. 7., *Nicomachean Ethics* *VU*, 3:1 47a; *VI*, 2:113a, *DeAnima III*, II:1143b. (cited in Green, 1980:xvi) where the conclusion of the argument is in the form of an "act", or here for the policy maker, "Do X." In such a case, even a well formed argument with "true" premises is no guarantee that a policy maker will take such an argument seriously (Miller and Safer, 1993). For the policy maker, who happens to be a philosopher of social science, let us say, the situation is even more desperate. Even with a fully specified model of the kind argued for by Glymour, the philosopher-as-policy-maker will quickly recall the possibility of radical under-determination (Quine, 1960). Conversely, if the model is so fully specified, from a god's-eye point of view so that all possible (even incompatible) models are somehow integrated into a meta-model, the situation for making concrete ("Do X") policy decisions becomes exponentially worse because of the complexity (and, most

likely, abstruseness) of the model. Ironically, if the super-model were to be "reduced" to a simple, parsimonious and elegant one, its "simplicity" would argue against its applicability to social policy concerns which now come to be viewed as "highly complex" and beyond the "simplicity" of the model.

The ideas above may be further related in a general way with Glymour's (1980) own notion of "bootstrapping." (Note 2) Even if we had a good, formal, and elegantly simple model (theory) of, say, the determinants of income inequality (see Miller, 1987:237-242 for arguments against the bootstrapping issue which, perhaps, *ought* to be the method-of-choice in showing how a causal-modeling framework is relevant to social policy-making). For instance, assume that the State Superintendent of Schools has evidence (in the form of standardized test scores used in the system) that there is a "strong" (e.g., $r = .70$) positive correlation between test scores and the SES of schools, i.e., SES and Achievement Test scores covary. From a bootstrapping perspective, we might suggest that any of the models, such as the ones noted above, could in conjunction with the evidence, be used to infer an hypothesis something like, "when controlling for IQ the relationship between SES and Achievement Test scores will be substantially reduced." Let us say this hypothesis is subsequently tested and IQ indeed does reduce the relationship between SES and test scores. This goes on in different ways and the theory is increasingly "confirmed"—in at least this sense of the elusive term (Achinstein, 1983). Bootstrapping would seem to be (if indeed it is increasingly supported) a desirable consequence for the policy maker; but in fact it is not.

II.

While desirable, an increasingly well confirmed theory is ordinarily of little pragmatic value for the policy maker. And this is not primarily due to the complexity or theoretical "simplicity" of the theory, nor to a lack of reliability searches, or problems of adequate statistical modeling, but rather to (1) the lack of a "logic" of policy implementation given the nature of the indicators in causal-modeling approaches themselves, (2) the lack of a clear "inference to the best explanation" model in which the issues raised previously—what counts as evidence and what evidence counts—become central, and (3) the lack of acknowledging the power of what we will call Ideological Proclivities in determining the "meaning(s)" of (1) and (2).

The major problem with using social science methods and modeling to *make* social policy is the failure to see that a type of "naturalistic fallacy" is involved, whereby the "is", in this case of *The Bell Curve*, as well as other attempts, is believed capable of being translated into the "ought" of policy making. To see this, some comments on the three points above. First, one of the most difficult issues policy makers confront is the implementation of indicators (as a part of *formulating* and *implementing* a policy) whose "status" may be epistemically sound but ontologically problematic. And, the problem is made worse as, paradoxically, we become more sophisticated in (as Glymour applauds) the use of such techniques as factor analysis which are used to reveal complex "underlying structures" or concepts. Thus, even with a non-problematic construct such as SES, the policy maker is confronted with the issue of how to implement its effects. That is, if SES is correlated with, say, IQ (a *problematic* construct), the policy maker must decide if (a) the *construct* can be changed or altered in such a way that those who do not have "enough" of it can obtain "more" of it or (b) if new social arrangements have to be constructed wherein those who have "enough" or "too much" of it can be persuaded to "share" it with others (e.g., social policy issues such as desegregation of schools through

"bussing") who have "less" of it, or those who have "enough" of it are kept away from those who do not because doing so (anticipating point three, ideology) is justified in some way. Now multiply this one variable case with the type of sophisticated causal modeling envisioned by Glymour and the problems increase accordingly.

The second issue related to the one just mentioned, is that of providing an "inference to the best *policy* decision "based on conventional notions of inference to the best explanation models (generally, Lipton, 1991). What is involved here is essentially the need for "rules" of inference which operate in two directions. The first involves the *creation* of a causal modeling theory which is the result of previous thinking and perhaps partial testing of the various "paths" in the model. The complete model is then tested further and claims about its efficacy *as* a model are put forth. In principle the model (or parts of it) can then be taken as the framework for developing a social policy, which then is tested. Both traditional "deductive" notions of theory use and Glymour's bootstrapping would fall under this approach. Now, even granting the "status" problems of the variables in the model as being capable of testing in some meaningful way, if such testing does take place the conclusions about whether the policy has "worked" are still problematic.

One problem of course is the adequacy of the testing procedures themselves, while another one is how the evidence stands in relation to the model and to the policy that is being evaluated. In another words, can the same evidence simultaneously constitute a best-inference explanation to both? In many cases, the answer to both is no. In the first instance, the way we often attempt to map the presumed *causal* relations of the model to the "real world" are contrived, or at best, constitute a partial mapping. As Glymour correctly points out, the way we "conditionalize" across different samples is crucial in what one's measures do or do not show. But the point we wish to emphasize is that such evidence, both in the "what evidence counts" and "what counts as evidence" senses, is not necessarily the evidence that counts *for* the policy. For example, the finding that SES and School Achievement do vary and are "explained" by IQ, let us say for the entire state of California, is more of a way of "confirming" this assumed relationship in the model than of formulating, implementing or evaluating a policy. That is, because of the nature of policy making as a form of practical argument ("Do X"), even a high correlation of model-specified variables is no guarantee of policy relevance in either the formulation, implementation, or evaluation phases of policy making. Yet such evidence may be strong confirming evidence for the model itself.

On the other hand, what counts as evidence might be given a broad definition for a given policy irrespective of any causal modeling considerations, or perhaps more accurately, *incidentally* of causal-model considerations. For example, the Superintendent of Schools in a state is aware that the "literature" is strongly supportive of a SES-IQ-School Achievement connection, and a similar pattern seems to be the case in her own school system. She formulates a specific policy in which she believes the only way to raise test scores (which are deemed "not acceptable") is to permit no one in teacher training programs with an IQ of less than 115; remove teachers who score below this; and significantly increase the salaries of present and future teachers who are or will be at this level. Additionally, what counts as evidence for the policy (in its formulation and implementation) may be a wide variety of "evidence" including previous empirical and non-empirical studies, reports, anecdotal descriptions, philosophical arguments, and so on. These same, or different, evidence sources may also be used to judge the "success" of the policy in its evaluation phase. In this scenario, which by the way actually often occurs, the inference-to-the-best-*policy* judgment is made on the basis of non-causal model based evidence as instances of the inference to the best explanation

(read "explanation" as "successful" policy). While all of these variations on the social policy-causal modeling theme are relevant in varying degrees to the policy making process, the most relevant one in our view is that of implicit or explicit ideological preferences. How this issue works, and how even Glymour is not fully aware of its power, will be described below. However, before this is addressed, some further brief reflections on the points above may be in order.

Although not addressed by him specifically, we have found some of the recent work by Searle (1988, 1995; also see Review Symposium on Searle, 1998) to be especially useful in situating the social science research-social policy issue. In his continuing analysis of intentionality, Searle (1983, 1998: 99-104) introduces the notion of "conditions of satisfaction," a phrase which refers to the possibilities of judging a large class of intentional states in terms of their propositional contents. Some intentional states such as beliefs and hypotheses can be judged as true or false according to what Searle refers to as their mind-to-world direction of fit. That is, these intentional states are supposed to reflect the way the world is in terms of an independently existing reality. On the other hand, intentional states such as desires and intentions have a different direction of fit: a world-to- mind direction. Here, the issue is one of trying to make the world correspond to what is believed about it (see also, Anscombe, 1959; Austin, 1962).

The interesting parallel to the policy making-social research issue is that the direction-of-fit problem is actually counterintuitive to what one would expect. If we look at Figure 1, Glymour and many social scientists would expect that the increased sophistication of, especially, causal modeling processes will increasingly yield a true mind-to-world fit [i.e., *A*]. And, indeed, while this may prove to be the case in some ontologically- realist sense, it comes at the increased cost of having to *demonstrate* that the world (in the policy making sense) *is* such, and, hence, we end up with *C*: trying to fit the world to (again, in terms of policy making) what we believe it should be like on the basis of what it is predicted to be.

		Perspective	
		Social Scientist	Policymaker
Fit	Mind-to-World	<i>A</i>	<i>B</i>
	World-to-Mind	<i>C</i>	<i>D</i>

Figure 1

On the other hand, the policy maker want the world to be like (b), but in trying to apply *A* to it, she must argue for *D*. Both groups start out as "realists", in at least a broad ontological sense, but end up as "idealists" in having to reconstruct the desired fit. What results is a type of "reversed intentionality" where beliefs become desires, and desires are fitted into the beliefs—a result where social policy which "fails" is not so much the fault of the model itself but, ironically, of its sophistication. The double irony is that a "simple" model, while "fitting" in both senses, may be rejected by both policy makers and social scientists for this very reason. There is, however, another factor that needs to

be addressed and we turn to this now.

III.

Glymour's article opening is entitled, "What went wrong...?" In effect nothing went wrong! By this we mean the critical dimension in trying to understand the relationship between social science causal-modeling and social policy is how the "variable" of ideological preference enters into the equation. The importance of "U" (p. 18) in Glymour's critique is not in some covert empirical variable influencing our model making but rather how model-making is interpreted by way of ideological preferences and proclivities. It is this "variable" that ultimately accounts for our constructions of social reality (Searle, 1995).

The ideological factor is a world-to-mind problem of fit and does, of course, go in both directions—those of social scientists as well as policy makers. Moreover, while the ideological frameworks of those above may be implicit or explicit, there is yet another "level" or group that comes into play here, namely those affected by the policy. What "voice" these individuals obtain from the policies that are usually imposed on them is a function of how well decisions affecting them are understood and the degree of political action garnered for or against the policy. Knowledge of how the ideological factor operates is further complicated by the fact that there are at least two methodological stances one may take to characterize this process—a variety of the mind-to-world problem. These possibilities are given in Figure 2.

Models

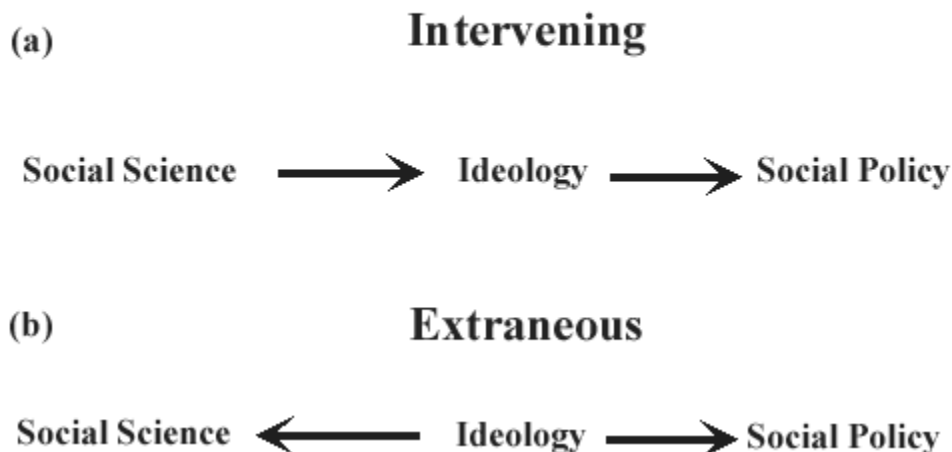


Figure 2

The categories of "intervening" and "extraneous" are meant to be used as they are in social research: an intervening variable as logically "fitting" between an independent and dependent variable, and extraneous, as a variable separately influencing the independent and dependent variables (Nachmias & Nachmias, 1981). For social research and policy, the intervening variable example suggests that an ideological stance is taken (by either social scientist, policy maker, on those directly affected) in such a way that one views it as being compatible with the social policy. That is, the ideology becomes

the *justification* for the policy; it is a filter which translates the findings into *acceptable* policy decisions. Thus, if one believes, as in the *Bell Curve*, that there are empirical data which clearly support cognitive differences among racial and ethnic groups, that belief system "intervenes" nicely between the research findings (and approach) and the policy subsequently formulated. In the "extraneous variable" model, the ideological belief system, let us say of the policy maker, is different because it admits of the possibility that the policy maker may *reject* the research findings and yet maintain the efficacy of a particular policy formulation. For instance, if SES differences are correlated with performance on standardized tests, one may reject that they have a hereditary basis and yet may find such results compatible with a "welfare state liberalism" or "educational progressivism" social policy which would support a variety of educational interventions. Moreover, even if the research indicated that racial or ethnic differences remained after controlling for SES, one could still argue that the meaning of SES is "interpreted" differently by different groups. Thus "income", for example, may be "equal" between two groups, but one group utilizes income to invest in "cultural capital" than the other, and it is this factor that makes the difference in test scores; again, an interpretation ideological compatible with the categories above.

We are not suggesting, in some simplistic fashion, that ideological commitments or preferences are always working as "biasing-filters", but only that they are an often overlooked factor in *explaining* how social policies are formulated, implemented and evaluated given social science research findings. Additionally, the ideological proclivities of all directly or indirectly involved in policy making produce a variety of conflation that are often overlooked in discussions of these issues. Thus, some feminist epistemologists (Tyson, 1998) see their particular agendas, and the social policies flowing from them, as being more (or only) compatible with "qualitative" research methods—what counts as evidence and what evidence counts is ideologically conditioned. In a similar way, entire ideological movements such as "constructivism" (Cobb, 1994, Von Glaserfeld, 1995), while not being overtly hostile to empirical methods, do come down on the side of "ethnographic" approaches.

How the ideological factor is prominent in Glymour's thinking can be made clear when he states (p. 28):

Sensibly read, much of the data of *The Bell Curve*, as well as other data the book does not report, demands a revived and rational liberal welfare state, but instead the book ends with an incoherent, anti-egalitarian plea for the program of right-wing Republicans.

We now know where Glymour stands ideologically, although it is an open question if his political preferences were "caused" directly by the evidence, *his* reading of it, or irrespective of both. It is probably the middle option of the above. On the same page (p. 28) he berates *The Bell Curve's* assumptions that the decline of the two-parent family is a factor in such things as low school performance. He may be correct in this, but his citing of Murray (1984) to the effect that two parent families are in decline in industrialized societies, does not tell us how or why the Murray evidence conforms to his own causal-modeling structures. Does the evidence in Murray adequately account for all the problems he has cited? If so, some passing mention of it could have been made.

Continuing on (pp. 27-29), Glymour makes a huge leap from the fact that Herrnstein and Murray favor some form of privatized schooling to the "fact" that we will end up with "Ku Klux Klan schools, Aryan Nation Schools... and more schools of ignorance, separation, and hatred will bloom like some evil garden, subsidized by taxes"

(p. 29). Before the quote here he uses the phrase, "The consequences are predictable." How poor Modus Ponens is still abused! Where is there any evidence that privatization has or will lead to such outcomes. There are several other instances in the remaining pages (pp. 29-30) of the article where Glymour does seem to be aware of what evidence counts or why it counts. For example,

- He favors neither more decentralization or privatization of schools but rather national standards, testing and funding.
- He favors schools that are always open for children from 1 to 17, that can serve as both centers of learning and safe havens, and says they are the "sane and comparatively economical way to create and sustain a civil society."
- He favors early intervention efforts as worthy and these *can* produce lasting effects (contrary Herrnstein and Murray's conclusions) if "teachers are paid reasonably." He also says not having his vision of infancy to young adulthood quality schooling will result in higher "opportunity costs" than the 100 billion per year cost he estimates.
- He believes "over credentialing" (carried out by colleges and universities) penalizes the potentially positive effects of various compensatory efforts (i.e., affirmative action programs).

Finally, Glymour gives us *his* complete policy vision (p. 30): "Here is an alternative vision, one I claim *better warranted by the phenomena* Herrnstein and Murray report: nationalized, serious, educational standards, tax supported day and night care, a living minimum wage, capital invested in systems that enable almost anyone with reasonable training to do a job well." He then concludes if policies advocated by such conservatives as Gingrich and Gramm are instituted, we will end up pretty much a nation like Honduras!

In brief, the "policy" recommendations Glymour is advocating are not substantiated explicitly by any evidence that would count in their favor. And if there were such evidence, he does not tell us of its adequacy in causal-modeling terms. Ironically, Glymour's strong support for national standards is very close to what Hirsch (1996) has recently, and somewhat persuasively, argued for—although we would not equate Hirsch with being politically liberal. But the most telling phrase, we believe, in all of this is the emphasized passage above; namely that from the same data presented by Herrnstein and Murray, Glymour draws quite different conclusions—certainly an interesting variant on the under-determination thesis.

Finally, so that we may not be misunderstood, we agree with almost all (except the Honduras slam!) that Glymour is advocating. We are just saying that you can't get there in the way the Glymour thinks you can. The "is" of causal-modeling processes in the social sciences will not translate in the "Do X" of policy making. If Glymour does not believe this, he ought to consider running for a local school board.

Notes

1. One may notice that the policy-making process involves at least these three stages. Each may have an independent or sequential relation to the issue of social science research findings as evidence.
2. Bootstrapping refers to the complexity of trying to adequately determine what evidence and what type of evidence properly applies to the testing of theories. The

"bootstrapping" means that the evidence is first connected with the theory and both, then, are used to deduce the hypotheses of the theory. The general issue is *how* theories are to be confirmed. Here, how *do* social science theories result in social policy?

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